

# LETTERS

FROM

*Dr. WITHERING, of Birmingham,*

*Dr. EWART, of Bath,*

*Dr. THORNTON, of London,*

AND

*Dr. BIGGS, late of the Isle of Santa-Cruz;*

TOGETHER WITH

SOME OTHER PAPERS,

SUPPLEMENTARY TO TWO PUBLICATIONS

ON

*Asthma, Consumption, Fever,*

AND OTHER DISEASES,

BY

THOMAS BEDDOES, M. D.



BRISTOL:

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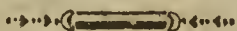
OXFORD

1871

THE

JOSEPH BLACK, M. D.  
 PROFESSOR of CHEMISTRY

In the University of *Edinburgh*.



DEAR SIR,

I HAVE always, since our first acquaintance, found gratification in avowing the respect I entertain for a character so eminent as yours for genius and candour. But your late adoption of Mr. LAVOISIER's system has greatly added to the force of this sentiment: And the recollection of so signal a proof, that neither years nor celebrity—the bane of vulgar minds—have had power to blunt your sensibility to truth, affords me greater pleasure than I should otherwise have felt in dedicating to you the following small collection of observations.

These observations principally relate to a subject of unspeakable importance; and one, in which our own countrymen are more deeply interested than almost any other portion of the human race.—The *invariable fatality* of Pulmonary Consumption is among those notions which observation and reading render most familiar to the minds of medical men. Many other persons are, however, by no means fully apprized of this melancholy truth. For “*Catarrhs*,” as a great physician has observed, “are sometimes mistaken by the ignorant for “*Consumptions*; or designedly called so by the crafty. “Hence they are *supposed* to have been occasionally cured.” Dr. Cullen used to mention in his lectures *one* instance of recovery from what he supposed to be a real consumption. But he professed himself unable to form any conjecture how this almost miraculous event was brought about; and of course he could not apply it to the benefit of other patients. I have heard of no other credible instance of recovery from well-ascertained consumption, except those mentioned in the following communications; yet the different practitioners whom I have questioned respecting the result of their experience must, I should imagine, have seen ten or, perhaps, twenty thousand patients. It may very safely be assumed that at least ninety-nine out of every hundred persons, ill of consumption, are cut off, notwithstanding the very earliest administration of the various compositions proclaimed by advertisements, or of the means advised in books, for their relief. Now what sort of remedies must those be, under whose ope-



ration nineteen patients die out of twenty, or even four out of five? Do they deserve the name of *remedies*? Credulity might, in this instance, take a lesson from the reply made to a boastful Pagan priest; who, in order to surprise a traveller into admiration of the power of his Deity, produced a list of the names of persons whom He had preserved from shipwreck, in consequence of the vows they had offered to Him; "very well," said the traveller, "so far, so good; now let me see the list of those who perished in spite of their vows."

If the means in use for the cure of consumption uniformly fail, the means of prevention are also lamentably deficient. Every body knows the disease to be dangerous; the signs therefore that indicate its approach commonly excite alarm, and, on their first appearance, few except the needy neglect to call in the aid of medicine, and many, doubtless, steadily pursue the directions they receive. Nevertheless, rich houses are every day discharging into the grave victims to this dire disease.

Delusion of every kind will, I imagine, on calculating its effects, be found injurious to society. The prevailing degree of persuasion, that Pulmonary Consumption has been and may again be cured either by quack medicines or by any other of the usual means, is obviously productive of two bad consequences; 1. As it enables a most pernicious species of impostors to fatten on the produce of fraud; and 2. As it renders physicians less active in seeking, and the public less urgent in requiring, an efficacious method of treatment. Are these evils outweighed by the common-place plea in favour of deception? Of this plea, which is suggested sometimes by real, and sometimes by affected humanity, I, for my own part, question the validity: in the first place, because I have observed phthical patients, under full assurance of their fate, suffer less than others, who have only suspected their danger (as they seldom fail to do), and who in consequence were agitated by incessant vicissitudes of hope and terror;—and, secondly, will not every sanguine patient, however firmly convinced that the *true consumption* is inevitably fatal, persuade himself that his own is not a case of *true consumption*?

Such, if I know myself, would be my opinion, were I a disinterested spectator of those scenes of domestic misery, which Consumption is every hour producing. The desire—a reasonable desire, I hope—of seeing my own project fully and speedily carried into execution, may render me more eager to dissipate any rival delusion. But I am confident, from the temper of the present age, and from several peculiar circumstances, that it will be tried in every possible form. Patients themselves, or their friends for them, will soon learn

to ask their medical attendants these two simple questions :  
 “ Have you had so much favourable experience of any other  
 “ method as to advise me to trust my life to it ? Do you know  
 “ the method, newly proposed, to be inefficacious ? ”—The  
 following testimonies, must also have the greater weight both  
 with the public and with the members of the medical pro-  
 fession, as they come from perfectly impartial and well-in-  
 formed persons. Many other of the most respectable prac-  
 titioners and improvers of medicine have expressed the most  
 earnest wishes for the execution of the design, as well as great  
 anxiety for further information. And were there no other  
 hope, those young men, to whom YOU communicate ardour  
 and information, would, I am sure, prevent this chance of  
 relieving otherwise irremediable misery from being lost to  
 mankind. The sooner, however, its pretensions are exami-  
 ned, the better ; in order either that the benefit may be dis-  
 seminated, or, in case of total failure, that ingenuity may  
 strike off in quest of other improvements. For who will  
 deny that the art of medicine needs improvement, while so  
 many and such frequent diseases remain incurable ?

The *pneumatic* practice is about to be introduced into one  
 hospital—another source of expectation. But an appro-  
 priated hospital, under the management of an able and  
 impartial physician, would soonest try this practice, and  
 improve it, if it be worthy of prosecution. Such an  
 establishment, with rooms proper for containing modified  
 airs, might be provided for a sum which, when set in compe-  
 tition with a small probability of greatly promoting the public  
 welfare, must appear contemptible. And an individual, who  
 from inexperience of the world, should suppose mankind in  
 general, open to conviction and alive to their true interests,  
 might imagine that the attention of the opulent would infal-  
 libly be arrested by considerations like the following :

“ Some exterminating maladies infest, almost exclusively,  
 “ the habitations of the indigent. But Consumption does  
 “ not confine its ravages within such narrow limits. Nor  
 “ has wealth yet been able to provide materials for erecting  
 “ a barrier, capable of resisting its invasion. The young,  
 “ the beautiful, and ingenious are its ordinary prey—and how  
 “ often have you to lament that it fastens upon the objects  
 “ of your fondest attachment ; after whose loss this busy  
 “ world will seem to you as a cheerless desert ?—I am aware  
 “ of the interest which a child, consuming by a slow decay,  
 “ must excite in the bosom of a parent. Full allowance,  
 “ however, being made for the effect of compassionate affec-  
 “ tion on the imagination, it will often appear, that the most  
 “ amiable individuals of a family are really singled out by  
 “ Consumption.” “ Self



“ Self-preservation comes in to second the dictates of  
 “ parental affection ; for it is certain that the number of  
 “ persons, who die of consumption at an advanced period of  
 “ life, infinitely exceeds the common computation.

“ In comparison with so unceasing and diffusive a calamity,  
 “ how inconsiderable are the effects of those epidemical dis-  
 “ orders, that occasionally excite so much consternation  
 “ among us ? Why then hesitate to accept the aid of Science,  
 “ when she offers agents endowed with *great* and *peculiar*  
 “ powers, advantageous in their application, and, as there is  
 “ some reason for supposing adapted to our necessities ? Is  
 “ a full trial of their efficacy too expensive ? At what rate  
 “ then do you estimate the chance of learning how to  
 “ preserve from otherwise inevitable destruction those whom  
 “ their understanding or disposition may have rendered your  
 “ pride or your delight ? How many times a larger sum may  
 “ you have to bestow without receiving in return any chance  
 “ of their preservation ?—But you have heard the project  
 “ vilified. So would a Panacea be. So was the Peruvian  
 “ bark ; and Inoculation ; and every great improvement of that  
 “ art, from which, according to its state, all in their turn shall  
 “ experience good or harm. Besides, are you sure that those  
 “ who pass this sentence are uninfluenced by prejudice,  
 “ pride, or the thirst of gold ? Recollect that to decry what  
 “ we do not understand is an obvious expedient of self-love ;  
 “ consider therefore whether the information of these men  
 “ is such as may enable them to judge from analogy, or  
 “ whether they speak from actual experience : For opinion  
 “ can have no solid base but in analogy or experience, since  
 “ an intuitive perception of the powers of nature is not  
 “ among the faculties of man. Authority, equal to any that  
 “ can be opposed, is adduced in favour of the proposal.  
 “ Many considerations concur to render it plausible. The  
 “ few trials, hitherto made, have answered beyond expect-  
 “ tation. There is nothing, for example, in the authentic  
 “ records of medicine similar to the case of *florid* consump-  
 “ tion related in one of the following letters. The relapses  
 “ serve but to render more evident the connection between  
 “ cause and effect. The same observation applies to the case  
 “ of putrid fever, related in another letter.”——

I flatter myself that the art of medicine will find great  
 resources in OXYGENE or VITAL air. Its powers, as far as  
 I have hitherto tried them, have exceeded my previous  
 conceptions. But as every substance, worthy of being re-  
 garded as a medicine, must be capable of doing much mis-  
 chief when misapplied, I am under some apprehension lest  
 mismanagement should bring this species of air into disrepute.

Whenever

Whenever it is administered to persons whose constitutions are not much reduced, nor their strength much impaired, it should at first be diluted with three times its bulk of atmospheric air; nor should this mixture be inspired above five minutes at a sitting three or four times a-day. The subjoined case of epilepsy, in which its effects did not correspond to our wishes, will serve to enforce this caution. Within these few days another confirmation of this rule has occurred to me: An asthmatic patient, finding great relief from atmospheric mixed with oxygene air, unadvisedly determined to attempt to subdue his disease at one attack. By largely using oxygene air little diluted, he brought on some singular symptoms, but, I hope, without doing himself permanent mischief. I do not enter into further particulars at present, as I shall probably have an opportunity of laying this case before the public in the words of the patient himself.

In several experiments with animals that had respired diluted oxygene air, I have found them upon immersion in water much more vivacious than similar animals that had breathed atmospheric air. Of these experiments I intend soon to give an account, together with a drawing and description of a chamber-apparatus for procuring and containing elastic fluids. In the mean time, it were to be wished that a number of persons would engage in this promising investigation. It might perhaps be determined, whether phthical patients vitiate the air more than persons in health?—whether asthmatic patients, during a fit, vitiate it less, as Mr. Chaptal, I think, asserts?—An instrument for measuring the capacity of the lungs in different people might easily be contrived; and such an instrument might possibly be useful as well as curious. But heads of inquiry will occur to any one who considers this copious subject.

“ Supposing the proportion of ingredients in the atmosphere to be that best adapted to the average state of health, “ is it not likely that there may be certain deviations from “ this state, where that fluid body contains too little vital air, “ and other deviations, where it contains too much?” Your encouragement of the inquiry, will, I hope, assist in furnishing the solution of a problem, which is certainly one of the most important in physiology and pathology.

I am, dear Sir,

Your affectionate Friend,

*Thomas Beddoes.*

*Hope-Square, Bristol Hotwells,*

*Dec. 24, 1793.*



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OBSERVATIONS on the Alteration produced in the AIR of  
PLACES where a GREAT NUMBER of Persons are  
assembled. By Mr. Lavoifier.

*THIS Paper is taken from the Memoirs of the Paris Society of Medicine, a Work that does not fall in the way of ordinary readers. It is valuable in itself, and intimately connected with the subject of Diseases that may be cured, or relieved by breathing different airs. I am indebted for the translation to the kindness of a Friend. The chemical terms are rendered conformable to the new French nomenclature, which did not exist in 1785, when this paper was read; and the degrees of Fahrenheit's scale are substituted in the place of those of Reaumur, but the weights and measures are French.* T. B.

MODERN Chemists have discovered that, besides the common respirable air, there is in nature a variety of fluids which agree with it in its most obvious properties. Like the air of the atmosphere, they are colourless, and so perfectly elastic, fluid, and transparent, that they would escape the Sight and Touch, if their resistance and the possibility of confining them, did not in many instances convince us of their existence. But though they bear a considerable resemblance to common atmospheric air in their external, or what may be called their physical qualities, yet they are found to differ essentially when chemically examined; viewed in this light, some are discovered to be nothing more than the ordinary alkalis or acids in a state of vapour; others are neutral substances of a very singular nature, and there are others again whose properties have not yet been ascertained. Accurate and profound researches into the nature of aeriform fluids, have shewn that they are indebted for their elastic state to the matter of heat, which enters into their composition: that all volatile substances whatsoever are susceptible of *Evaporation*, and are transformed into a species of air by a certain quantity of heat: that the upper surface of the mercury in the barometer, for example, being at its mean height, (or about 28 Paris inches above that of the mercury in the basin) water assumes an aeriform state at the temperature of  $212^{\circ}$  degrees of Fahrenheit's thermometer; and Spirit of Wine at that of  $75^{\circ}$ : that these fluids, thus rendered elastic and aeriform, are capable of being confined under glass bells, or other receivers: that they may be transferred from one vessel to another, and

subjected to all the experiments that can be made on permanently elastic fluids. This aeriform state, or that of an elastic fluid, is nothing therefore but a modification, and the words *air* or *gas* are mere generic expressions characterising a certain class of bodies, but not appropriated to any particular species. The common air of the atmosphere is consequently only an individual of this numerous class.

These general considerations might induce us to consider the atmosphere not as a simple but as a compound substance: it may be a mixture of all the various substances capable of assuming the state of air at the degree of heat, and under the pressure in which we live. Experience has confirmed this conjecture which was suggested by analogy. Chemists having ventured to analyse the air of our atmosphere, they have succeeded in discovering that it consists of about 27 or 28 parts in 100 of an air perfectly fit for the purpose of respiration, and now known by the name of oxygene air, and of 72 or 73 parts of a mephitic fluid, absolutely incapable of supporting the combustion of Bodies, or the respiration of Animals, which has lately been denominated azotic air.

In the proportion just mentioned, of 72 parts of azotic air, to 28 of oxygene air, the number of cubic inches occupied by each in a cubic foot of the common atmosphere, is found to be as follows:

Oxygene air,	-	-	484 inches,
Azotic air,	-	-	1244

Total 1728 inches = cubic foot.

I have found by a number of experiments, of which I shall hereafter give an account, that when the barometer is at the height of 28 Paris inches, that is to say, at its mean height, and the thermometer at 52 degrees, the cubic foot of atmospheric air weighs

	-	-	-	-	-	oz.	grs.	grs.
						1	3	3
The weight of a cubic foot of oxygene air is						1	4	0
And of a cubic foot of azotic air	-	-				1	2	48

Hence it follows that a cubic foot is composed

		Inches.				grs.	grs.
Of Oxygene air,	-	-	484	} weighing {		3	26
Azotic,	-	-	1244			7	49
			<hr/>			<hr/>	
			Total 1728			1	3 3

Amongst the different substances of which the atmosphere is composed, none besides oxygene air is essential to respiration: the azotic air contributes nothing towards it: so that, in fact, any other mephitic fluid might be substituted in its place; and, provided this substituted fluid possesses no irritating or deleterious quality, and is combined with oxygene air in the proportion



proportion of 72 parts in 100, such a mixture would constitute a fluid equally salutary, and respirable with the common air of the atmosphere.

Such is the knowledge of the composition of the air we breathe, which the Science of Medicine has derived from Natural Philosophy and Chemistry.

But what are the changes produced in air thus formed in the various circumstances of Life? what the influence of these circumstances on the organs of respiration? what diseases in the Animal Œconomy may hence arise? and what are the methods of preventing or remedying them? To answer these questions is the object of my present undertaking; and of these I shall give an account to the Society from time to time in different papers.

It is a fact which has been long known, that respiring animals live only for a given time in a given quantity of atmospheric air; they soon become faint, and sink into a kind of slumber: this slumber, though composed at first, is succeeded by great agitation: the respiration becomes quick and difficult; and the animal expires in convulsions. These events succeed each other with greater or less rapidity in proportion to the quantity of air in which the animal is confined, and in proportion to its general bulk, and to the comparative size of its lungs: The vigour of any given animal may likewise contribute somewhat to prolong its existence for a short period, but in general it may be considered as an established fact, that a man cannot subsist longer than an hour in a quantity of air equal in bulk to five cubic feet.

In order to obtain an adequate idea of the species of injury which the air sustains by being respired, I introduced a Guinea-pig under a glass bell inverted upon mercury, which contained 248 cubic inches of oxygene air. I suffered the animal to remain in these circumstances about an hour and a half; at the end of which time, I removed it, by the same way in which it had been introduced, by passing it through the mercury. I did not perceive, that in either of its passages it had been in the least injured.

In order to facilitate our future reasonings, I shall suppose that the quantity of oxygene air in which the Guinea-pig was confined, amounted to a cubic foot, or to 1728 cubic inches, and I shall reduce by calculation all the results of my experiments to this standard.

When the Guinea-pig was withdrawn from under the bell, the 1728 cubic inches of oxygene air were found to be reduced to  $1672\frac{3}{4}$ ; the diminution of bulk was consequently  $55\frac{1}{4}$  cubical inches; in the mean time there were formed  $229\frac{1}{2}$  cubic inches of carbonic acid air. Of this fact I satis-

fied myself by introducing a quantity of caustic alkali into the bell; the air remaining after this operation was perfectly pure oxygene air.

Considering these portions of air with respect to their weight, we shall have for the quantities remaining under the bell after the animal had been withdrawn, the following proportions :

	oz.	grs.	grs.
Oxygene air, - -	1	2	$1\frac{3}{4}$
Carbonic acid air, -	0	2	15
	<hr/>		
	1	4	$16\frac{3}{4}$

In this experiment the air appears to be diminished in bulk about 1-32d part : but its absolute weight augmented : hence it evidently results, 1st. That the air derives from the lungs during the act of respiration, a portion of carbonic acid air : But it must be remarked that this augmentation of weight which appears to be only 21,87, is in reality much more considerable than it appears to be at first sight. The experiment which I have just related, produced no more than  $229\frac{1}{4}$  inches of carbonic acid air ; now according to very exact experiments which I have described elsewhere, 100 parts of carbonic acid air in weight are composed of 72 parts of oxygene air, and 28 of charcoal. The  $229\frac{1}{2}$  inches of carbonic acid air obtained in this experiment contained therefore of

	grains.
Oxygene air, - -	114,84
Carbone, - -	44,66

The 114,84 grains of oxygene air amount in cubic inches to  $229\frac{1}{2}$  inches ; if then no more oxygene air had been employed than was necessary to form the carbonic acid air, the quantity remaining after the operation should have been  $1728 - 229\frac{1}{2} = 1498\frac{1}{2}$  ; it was only 1443 2-3ds. the deficiency is = 54 2-3ds.

It is evident from this statement, that independantly of the portion of oxygene air which has been converted into carbonic acid air, another portion of that which has entered the lungs has not returned in an elastic state ; and it follows that one of these two effects takes place during the act of respiration ; either that a portion of oxygene air is united with the blood, or that it is combined with a portion of inflammable air, and composes water. I shall discuss in other papers the reasons which may be adduced in favour of each of these opinions. But allowing, (which there is some reason to do) that the latter is the preferable supposition, it is easy, from the above experiment, to determine the quantity of water which is formed during respiration, and to ascertain the quantity of hydrogene extracted from the lungs.

In fact,



In fact, since to produce 100 parts of water it is necessary to employ 85 parts by weight of oxygene air, and 15 of hydrogene gas, it follows that the 54 2-3ds. inches of oxygene air which have not been accounted for, must have formed  $32\frac{1}{4}$  grains of water, and that 4 5-6ths. grains of inflammable air have been disengaged from the lungs of the animal. The same experiment repeated in common air, affords similar results: a diminution of the bulk of the air: an augmentation of its absolute weight: a formation of carbonic acid air, and of water: a disengagement of carbone, and of a small portion of inflammable gas from the lungs: but the azotic air which remains, and which mixes with the carbonic acid air, and with the portion of oxygene air not entirely consumed, renders the result more complicated. At the time therefore, that the respiration of the atmospheric air has been continued as long as may be, and animals can no longer remain in it, except at the risque of losing their lives within a few seconds, it is found to be composed of nearly the following proportion in each cubic foot, I say nearly, for great variations are observable in these circumstances, and particularly in the quantity of carbonic acid air. A cubic foot contains therefore in these circumstances,

Of oxygene air,	-	-	173 inches.
Carbonic acid air,	-	-	200
Azotic air,	-	-	1355

Total 1728

Which gives in weight,

			oz.	grs.	grs.
Oxygene air,	-	-	0	1	14
Carbonic acid air,	-	-	0	1	66
Azotic air,	-	-	1	0	26

Total 1 3 34

I ought to take notice, that these results were determined by means of respired air after it had been cooled, and had deposited the superabundant humidity which it had acquired in passing through the lungs. Air thus exhausted by respiration, proves that the limits within which it is possible to vary the proportions of oxygene and azotic, in order to produce respirable air, are not very extensive, and that consequently it is no wonder that the air should be found sensibly injured in a great variety of circumstances. In the experiment made upon the Guinea-pig confined in oxygene air, which I have just related, I perceived that the animal suffered considerably towards the conclusion. It is however evident, that in this case a very small portion only

was absolutely vitiated, that is, converted into carbonic acid air, and that there remained of oxygene air a quantity much more than was necessary to constitute a salubrious air. This circumstance had been already observed by Dr. Priestley, but the object which I propose in this paper, required a repetition of a part of his experiments. My operations were generally performed upon Guinea-pigs. The oxygene air which I made them breathe was nearly pure; and did not contain above five or six parts of azotic air in 100 of the whole portion. Now though these animals lived much longer in a certain quantity of this air than they would have done in an equal quantity of the air of the atmosphere, they perished long before it was completely vitiated, while another animal of the same kind introduced into this vitiated air did not appear, for some time at least, to suffer any considerable inconvenience. It was not therefore for want of respirable air that the animals perished; it was rather owing to some pernicious quality in the oxygene air, a proof that the admixture of a certain portion of azote with oxygene is required to render it salubrious. M. Bucquet, whose name at this moment must renew the public regret, assisted me very kindly in some of these experiments, and we opened together the animals which had fallen victims to our researches; they all appeared to have died of a burning fever or some inflammatory disease. Their muscles, upon inspection, were found to be very red; the heart livid and full of blood; especially the right auricle and ventricle: the lungs were but little inflated, but were red even externally, and gorged with blood. A truly salubrious air therefore is composed of an adequate mixture of oxygene and azote: and it is of consequence to respiring animals, that this proportion, which is commonly 28 parts of oxygene to 72 of azote, should never vary in any considerable degree. This difference however is observable, that when the oxygene is superabundant, the animal only suffers severely: when it is deficient, the consequence is immediate death. Since, therefore atmospheric air supports life for a certain period only, and since it becomes the more vitiated the oftener it is respired, we may venture to conclude that the wholesomeness of the air must be more or less diminished in all public places, in hospitals, and wherever a number of persons are assembled; especially if the air circulates slowly or with difficulty. I thought it of some consequence to determine to what extent this vitiation could possibly be carried, for which purpose I chose the lowest ward in the General Hospital, which appeared to me more crowded and unhealthy than the rest. I went thither at day-break, I was admitted the instant the door was opened,

and



and filled two phials with the air of the room ; one I filled from the lower part of the room nearly on a level with the floor, and the other from the upper part, or as near as possible to the cieling. The former of these two portions of air, or that which was taken from below, was but little vitiated ; it contained in two portions, in bulk,

Of Oxygene air,	-	-	25
Carbonic acid air,	-	-	4
Azotic air,	-	-	71

---

100 Parts.

The air taken from the top of this ward had suffered much greater injury. It contained,

Of Oxygene air,	-	-	$18\frac{1}{2}$
Carbonic acid air,	-	-	$2\frac{1}{2}$
Azotic air,	-	-	79

---

100

Atmospheric air taken the same day in the open air contained,

Of Oxygene air,	-	27
Azotic air,	-	73

---

100

I attempted the same experiments on the air of a theatre. The French comedians were at that time in the palace of the Thuilleries, and I performed my operations in that building. I chose a day in which the number of spectators was unusually great, and taking with me two phials full of water, I emptied one at the top of the theatre, in a box which had been kept shut during the whole of the performance, and the other at the bottom of the pit, a few moments before the conclusion of the play. It is easily conceived, that this second part of my operation was attended with some trouble and difficulty : the least appearance of any thing extraordinary, would have occasioned disturbance in the pit, and might have put a stop to the performance. I was obliged therefore to be satisfied with coming in gently a few moments before the end of the play, and placing myself near the sentinel, whom I had informed of my scheme, emptying my phial in that awkward situation. But the air which I thus obtained was taken too near the door, and the water through which it passed in order to enter the phial, must have absorbed a portion of its carbonic acid air. On this account, the experiment did not give me any results sensibly different from those made with the external air ; but this was not the case with the air collected at the top of the theatre. In 100 parts of this air there were found

Of

Of Oxygene air,	-	21
Carbonic acid air,	-	$2\frac{1}{2}$
Azotic air,	-	$76\frac{1}{2}$

---

Total 100

Whence it is evident that the quantity of oxygene air had been diminished in the proportion of 27 to 21, or nearly one fourth. It is to be wished that these experiments could be repeated more at large and with a more convenient apparatus. The washing of the air, at the time of collecting it, should be above all things avoided. This might easily be effected by means of tin pipes communicating from the outer to the inner parts of the building, to whose extremities should have been previously fitted balloons exhausted by the air-pump.

In this manner it would be easy to procure a quantity of air sufficient to determine its specific gravity: the experiments might also be conducted on so large a scale as to render even minute differences very sensible; and they might be repeated a sufficient number of times to render the inaccuracies which in all delicate experiments are unavoidable, nearly evanescent, and make them compensate one another. Such experiments cannot be well carried on except under the sanction of Government; but undoubtedly we should derive from them valuable information with respect to the construction of theatres, hospitals, and every other building, in which people assemble in great numbers.

However imperfect my experiments may be, we may collect by comparing them with others made on a smaller scale, under glass vessels, that the air of the atmosphere which is originally composed of only two fluids, or very nearly so, is composed of three in all places which contain numerous assemblies; in consequence of the conversion of a part of the oxygene air into carbonic acid air: that these three fluids are not mingled in equal proportions in every part of the room, but on the contrary tend to arrange themselves according to their specific gravities: that the azotic air, as being lighter and favoured by the heat which expands it, naturally mounts upwards; and thus a species of circulation is produced which supplies the place of the mephitic air, which escapes at the top, with fresh air flowing in from the lower avenues.

This circulation takes place more or less in every theatre; and frequently in spite of the architect who directed the construction: unless this was the case, unless the air was thus renewed, the spectators would be exposed to the most fatal accidents long before the conclusion of the performance. To convince ourselves of this truth, nothing more is necessary than to take the example of a theatre, suppose of 30 feet long,



long, 25 feet wide, and 30 feet high. A room of these dimensions would be equal in bulk to 22,500 cubic feet, and might contain about 100 spectators: now since each person consumes, as I have mentioned above, about five cubic feet in an hour, it follows that the air of the theatre (if it were not renewed) would be rendered completely mephitic in four hours and a half: and it is likewise probable, that the greater part of the spectators would be seriously incommoded or even perish before the end of that period.

The same calculations applied to low and close places of resort, of which I could mention many instances, will explain how it happens that on crowded days the attention of the audience cannot in such places be kept awake above two or three hours, where a mechanical impatience is brought on by a certain uneasiness and physical anxiety, of which it is difficult to discover the cause. In such circumstances unfortunate is the reader to whom have been allotted the last moments of the sitting; an interest in his subject is no longer communicable to his audience: he is no longer listened to with complacency, or even with attention: and he receives none of those tributes of applause or gratitude, which in more favourable circumstances he had a right to expect.

When I began the present paper, my intention was to have given some account of the various species of injury which the air is capable of receiving in the ordinary circumstances of life. But I perceive that I have as yet done no more than sketch one point in the plan which I had adopted, and am obliged to refer to a second dissertation the remarks I have to make on the vitiation of the air produced by the burning of lamps, wax, tapers, candles, coal, by fresh plaster, oil-painting, &c. but as this part of my work is nearly finished, I shall soon have it in my power to present it to the Society.

There will remain to be treated of in a third paper, atmospheric air considered not as an elastic fluid susceptible of decomposition, but as a chemical agent capable of taking up, in the way of solution, miasmata of various kinds. It is somewhat alarming to consider how often in a large assembly, the air which each individual breathes, has passed either wholly or partly through the lungs of all those who are present. It must take up in each case exhalations more or less putrid. But of what nature these exhalations are: to what degree they vary in different subjects: in age or youth: in health or sickness: what diseases we are capable of receiving by this mode of communication: and what precautions may be employed to neutralise or destroy the dangerous influence of these miasmata—there are none of these subjects which may not afford ground of inquiry, and surely there are none

of more importance to the human race. While every art is advancing rapidly towards perfection ; the art of living with comfort in society, of preserving in health and vigour persons obliged to meet in large assemblies, of rendering cities and great towns healthier, and the communication of contagious disorders less general, is unfortunately yet in its infancy.

The immense labour which might be founded on this important object, must be undertaken by Societies of learned men only ; no individual can flatter himself that he possesses knowledge sufficient to complete without assistance, a plan so complicated and extensive ; and it is from reliance on the advice, the information, and the assistance of this Society, that I have now undertaken to cultivate some few portions of this immense field. §

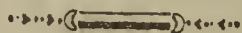
§ Mr. L. has, I believe, published nothing further on this important subject : And his incomparable talents are, I fear, now lost to Science and Humanity.

T. B.

## LETTER

*From Dr. WITHERING*

TO DR. BEDDOES.



DEAR SIR,

THE design you have conceived is an important one ; the philanthropist cannot but be interested in its success ; the physician must rejoice at the probability of learning how to cure or essentially alleviate a frequent, a cruel, and an hitherto hopeless disease ; philosophers will urge you to proceed, from a conviction that should you fail in your higher aims, you must extend the boundaries of science, and throw new light on the laws of the living machine ; and should your endeavours ultimately be crowned with success, and the most amiable, not to say the most beautiful individuals of our species, be thus snatched from a premature fate, numerous private families will be indebted to you for their greatest comforts, and Society at large for its brightest ornaments. It is from such considerations as these, as well as at your particular desire, that I now am about to communicate to you such observations, as have occurred during many years' attention to the phthisis pulmonalis ; but I must con-

fine



fine myself to those circumstances which more immediately tend to support or to invalidate your opinions, otherwise I should write a volume instead of a letter.

*Catarrhs, causes of Consumption.*

Young people themselves, as well as those who have the direction of them, cannot have it too strongly enforced upon their minds, that a *cough* merely the consequence of a cold, ceases of itself in eight or ten days; that if it continue beyond that period, there is danger that a consumption may be the consequence. Bleeding, spare diet and the other usual modes of obviating inflammation should be immediately pursued untill the cough shall entirely cease; and particularly bleeding by leeches, or cupping on the part where any pain shall be felt in the chest.

*Causes of Catarrhs. (Observations p. 156, &c.)*

A sudden change from cold external air to that in a heated room, is certainly a much more frequent cause of inflammatory affections of the lungs, &c. than has hitherto been generally supposed; it is I believe the most general cause, but surely it is not the only cause of taking cold! I am persuaded that a sudden transition from a warm to a cold apartment or to a stream of cold air, will produce this effect. We do get colds in Summer when no fires are lighted in our sitting rooms, though not so frequently as in Winter. Horses and cows get colds, though they never experience much sudden change from cold to hot in the temperature of the air they breathe, whilst the dog, who from the temperature of the coldest seasons instantly on entering the house, lies down close the hottest fires, and vice versa, seems little if at all liable to catarrhal affections. Horses sometimes die consumptive, cows often; dogs I believe never.†

*Kinds of Consumptions. (Observations p. 112.)*

The different kinds of consumptions should be better distinguished than they have been; not only as influencing prognosis, but as directing to a more successful practice. I agree with you that patients have reaped no advantage from the prevalent idea that most consumptions have a scrophulous origin. One species you have happily named the *florid*, and it is readily distinguished. There exists also a *truly scrophulous* consumption, but it is a rare, and not an incurable disease, if the treatment be properly adjusted to its nature; but the treatment which I have repeatedly found successful here, would only hasten the florid consumption to

B 2

its

† I do not here forget the epidemical contagious catarrh or influenza, to which Dogs are subject.

its fatal termination †. When the scrophulous consumption cannot be traced by any known family disease; or by the more obvious symptoms in the constitution of the patient, it may sometimes be ascertained by knotted cords of lymphatics running down the neck, and dipping under the clavicle into the chest.

*Substance of the Lungs destroyed.* (Observations p. 146, letter p. 25.)

The existence of this fact is not disputed, but it must be a very rare occurrence. Would not the falling in of the ribs in the case you mention, be equally explicable on the supposition of a caries induced in their bony substance from a disease in the pleura? Or might not the increasing debility &c. of the patient be alone sufficient to produce such an appearance, which often occurs in ricketty children? At one period of my life I had opportunities of accurately examining the Lungs of many who died consumptive, but I never met with any thing like the destruction of them. It was once my intention to have given these observations to the public, but the utility of that design was fully answered by the publication of the works of the late Dr. Stark, because nothing that I had observed, had escaped the attentive searches of that ingenious and indefatigable man, by whose early death science was deprived of one of its most active votaries.

#### *Who exempt from Consumptions.*

It is a prevalent opinion that the workmen employed about Limekilns never become consumptive; and it is usual for the affected with the disease, to repair to ignited kilns to breathe

† Medical practitioners will probably be surprised at this passage. Having had an opportunity of conversing with Dr. Withering since his letter was written, I requested an explanation. He informed me that the practice he had found successful in what he considers as the truly scrophulous Phthisis, is peculiar to himself: he mentioned to me what it was, but desired I would not anticipate the account of it he himself designs to publish.

Not many weeks before his sudden death, the late Mr. Benjamin Colbourne, of Bath, told me that he had discovered a medicine which he had reason to believe not less efficacious in certain diseases of the urinary passages than his aerated alkaline water is in calculus. The patients whom he had treated complained of difficulty of retention of urine, which often came away involuntarily in small quantities with a sense of irritation in the urethra, and was fœtid and alkaline, as he shewed me by dipping into some of it paper tinged by litmus, and afterwards reddened by an acid. The effect of his medicine was to remove the above-mentioned distressing symptoms, and to change the quality of the urine, as he also shewed me by the effect of the urine of another patient who had been under his care for some time, upon test paper. The urine of this patient was distinctly acid, and not more offensive than ordinary urine.--His design was to try his method in a few more cases, and, if he was successful, to publish it. He had never communicated it; I am told that no account of it has been found among his papers. I hope however that further search will discover his preparation; otherwise the loss to humanity will be truly deplorable.

T. B.



breathe the vapour issuing therefrom. This rude mode of administering atmospheric air deprived of part of its oxygen and combined with a portion of carbonic acid, has not under my observation ever cured a patient, but still I am disposed to believe that opinions generally prevalent have some sort of foundation. In looking about for the causes which promote or retard the frequency of consumptions, different situations and occupations become of course objects of my attention; and the only classes of men I have yet observed exempt from the disease, are butchers\*, and makers of catgut. They both pass much of their time amidst the stench of dead animal matters, the latter very much so; the former live chiefly on animal food, and are much exposed to the inclemencies of the seasons, whilst the latter live as other manufacturers, and work under cover, in close and rather warm buildings. These people are always sleek, often fat, and the rosy bloom of health adorns their cheeks. These facts but ill accord with our theoretical notions of putrid diseases.

*Progress of Consumptions stopped. (Obs. p. 113.)*

The effect of pregnancy in arresting the progress of consumption has long been known, but it was reserved for you, Sir, to turn this remarkable fact to advantage. Should your idea concerning the effect of the impeded action of the diaphragm stand its ground, the application of a compressive bandage upon the abdomen cannot fail to present itself to your imagination.

But the progress of consumption is also stopped by insanity. This is a circumstance well worthy your attention: A young woman in the last stage of phthisis suddenly became furiously insane. After three months the insanity ceased, the phthisical symptoms returned, and she died in a few weeks.

A young gentleman whose father died consumptive, consulted me about a troublesome cough, pain in his chest, hectic fever and emaciation. I had no expectation of his living, but wished him to winter in a warmer climate: on his return the following spring, the phthisical symptoms had no existence, but there was an unusual oddity in his manner, which very shortly shewed itself in a confirmed insanity. For several weeks he was furious, but that state gradually gave way to an abstracted melancholic cast: after some years he grew more comfortable, and so continues, but is far from well. ¶

B 3

Diet

\* Nevertheless, I have at present under my care a Butcher from Wrington, in Somersetshire, who has been for some time in a true Consumption. He is much relieved and entertains sanguine hopes of recovery. T. B.

¶ I was apprized of the suspension of Phthisis by insanity. It is noticed in Cullen's *First Lines*. I have myself mentioned a case where the pneumonic symptoms and quick pulse of Phthisis were suspended by anasarca. Dr. Percival

*Diet of the Consumptive.*

Every body has seen the inefficacy of milk, fruit, and a vegetable diet, with more or less abstinence from fermented liquors. For three successive years the opposite method was pursued in a great number of cases, by a practitioner within the sphere of my observation. His patients were supported upon animal food, strong gravy broth, and porter or port wine. According to the most candid judgment I could form, these great variations in diet had no essential effect upon the disease; and then it was that my hopes of finding a cure for it first forsook me—but you, Sir, have revived these hopes.

*Vitriolic acid.* (Obs. p. 135.)

In the florid consumption, and in hæmoptoe, the use of this acid has been generally approved; but I think its effects are very problematical. The patients generally like the medicine at first, but I have repeatedly observed that in a few days, it has occasioned an increase of oppression, a straighter cough, more heat, and if persisted in, an hæmoptoe, though none had appeared before.—This also favours your theory.||

*Carbonic acid Air.* (Obs. p. 128.)

In the case which I saw perfectly cured by means of this air, and which I communicated to Dr. Percival many years ago, the expectorated matter was very copious, and very offensive. It was with a view to correct this fœtor, and by that means to diminish the hectic fever, that I thought of directing its use. I found the patient in the state just now mentioned, and had the satisfaction of seeing her cured. Further and more mature observation has long since convinced me, that this was a case of vomica, and not a true phthisis. Accordingly I took the first fair opportunity of confessing my error, as may be seen in the appendix to my account of the foxglove. But though my hopes founded on this first trial proved deceptive, I am still very much deceived if the inspiration of carbonic acid air has not greatly prolonged the existence of many truly phthisical patients. My mode of using it is more effectual than you may have supposed. I order the patient to sleep in a small room, take care to have the chimney on one side of the bed, and place an earthen vessel which will contain two or three gallons on the

cival relates a case where it might at first sight appear that *Phthisis* was relieved or removed by *hydrocephalus internus*; but a careful perusal of the narrative of this eminent physician will, I think, satisfy the reader that the symptoms of the former disease only yielded as the powers of the sensorium were gradually destroyed. (Med. Facts and Obs. I. 131.) T. B.

|| Dr. Withering has since informed me of a case where Spitting of Blood and Stricture of the Chest repeatedly succeeded the use of this acid. His observation, if well founded, is of the greatest importance in practice. T. B.



the opposite side, on a level with the pillow. Things are so managed, that the effervescence goes on slowly, and continues for great part of the night, the vapour as it rises passing over the patient's bed. If the sick are so ill as to be confined to the house, the same process goes on through the day. You will probably find other aeriform fluids better adapted to the cure of the disease, but I think you will observe good effects from this, particularly when the expectorated matter is foetid.

### *Vapour of Gums and Rosins.*

Many people are persuaded that consumptive patients have found good effects from inhaling the vapour of resinous or gummy resinous substances. The powder of these substances is directed to be sprinkled upon a fire in a chafing-dish, and the patient inhales the vapour as it rises. But here a question presents itself, whether the benefit should be attributed to the vapour of the medicine, or to that of the burning charcoal? Japanners are constantly breathing the vapours of resinous substances, but I never could observe that they were more or less subject to phthisis than others; casters of fine brass work very often die consumptive, much more so than any other set of artists in Birmingham. They dust their moulds with powdered rosin, the vapour of which rises copiously when the melted metal is poured in. But the mischief can hardly be attributed to this vapour, otherwise the Japanners would be affected; nor yet to the flowers of zinc, which are copiously diffused through the work-shops, because the casters of large brass work are not peculiarly liable to become consumptive. I suppose the Phthisis in these instances to be caused by the mechanical action of the powdery matters which float in the air in great quantities in these fine casting shops, and are necessarily taken in with the breath. Whilst flints for the potteries were pounded in mortars, the people so employed universally died consumptive, and the grinders of needles now often experience the same fate. \*\*

### *Effects of Diet on Respiration. (Letter, p. 12.)*

The experiments you wish for on this subject have in part been made. The late Mr. Spalding, who did so much in improving and using the diving-bell, was a man of nice  
B 4 observation,

\*\* Linnæus, or Ullholm, mentions a very curious experiment on the penetrating quality of this powder. *Quanto vitio pulvis lapidosus pectus oneret, apud Orsenfes Dalecarliæ videre licet, qui ex teneriori lapide arenaceo cotes suas rotatiles secant, et ante annum tricesimum Phthisici plerumque moriuntur. Quin et lapidæ Stockholmienses, tantum non omnes, aut calculo pulmonum, aut phthisi, aut hæmoptysi enecantur; quanquam pulvis ille tenuis lapidosus adeo penetrabilis est et volatilis, ut vesica urinaria, inflata suspensaque in officinis illorum post exactum annum, aliquot scrupulos pulveris tenuis lapidosi intus continere apprehendatur. Amoen. Acad. viii. 159.*

observation, and had he not fallen a sacrifice to the negligence of drunken attendants, would have thrown much additional light upon more than one branch of science. He particularly informed me, that when he had eaten animal food, or drank fermented liquors, he consumed the air in the bell much faster than when he lived upon vegetables, and drank only water.†† Many repeated trials had so convinced him of this, that he constantly abstained from the former diet whilst engaged in diving.

*Carbonic Matter.* (Letter, p. 68.)

Its effects upon living animals are yet but little known. Many people mix it with the food of their poultry, and think it contributes to fatten them. This is much in favour of your opinion that it does absorb oxygene in the heat of the animal stomach.—

These, Sir, are such remarks as occurred to my mind on reading your observations on consumption, and reflecting on the many ingenious ideas you have suggested. I shall be happy if they can be made subservient to the great cause in which you have engaged. An individual so occupied has a right to claim every assistance which his brethren can afford him: mankind have much to gain, and nothing to lose by such enquiries.

I remain, dear Sir, your's,

*Wm. Withering.*

To  
Dr. BEDDOES.

P. S. In constructing the apparatus I have no doubt but you will contrive to ballance the air-vessel inverted in water, so that the patients may inspire perfectly at ease. The resistance given to inspiration by the column of water, low as it is, in Mudge's inhaler, is so great that even a healthy person cannot long persevere in breathing through it, and I have never seen a patient use it so as to draw the air through the water as its author intended should be done.

†† I had inferred that "the faculty of living in air of a reduced standard is impaired by the influence of spirituous stimulants." This observation of Mr. Spalding, which seems equally new and interesting, adds much probability to the opinion. I intend to ascertain whether it be true or not, by direct experiments on animals.

T. B.

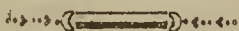
LETTER



## LETTER

*From Doctor E W A R T*

TO DR. BEDDOES.



DEAR SIR,

BATH, *November 14, 1793.*

I CAN have no objection whatever to comply with your request of stating to you in writing such of the particulars, as my memory distinctly retains, concerning the two cases of Phthisis Pulmonalis, in which I have employed the inhalation of mephitic air, with seeming advantage; and I give you leave to make what use of them you please. I am sorry however, that not having kept a regular journal of the cases alluded to, I must now confine my observations to general circumstances, and to their general result.

I accompanied the late Hon. Col. Cathcart, when he sailed from England in the year 1787, on an embassy destined for China. This Gentleman had from his infancy been subject to frequent and alarming pulmonary complaints; and at the period above-mentioned, being then 28 or 29 years old, he was threatened by such serious symptoms of Phthisis, that little hope was entertained of his recovery but from the effects of a sea voyage to a warmer climate. There was some prospect of this hope being realized, during the first part of the voyage; but after passing the Cape of Good Hope we were forced into a high Southern latitude where the cold was intense, and in which all the former symptoms of Phthisis returned upon him with redoubled violence. An almost incessant cough, a copious expectoration of matter, judged both by its appearance and smell to be of a purulent nature, and mixed occasionally with streaks of blood, a fixed pain in the breast affecting his breathing, together with a rapid emaciation and hectic fever, left no doubt of the confirmed and dangerous form of the disease. All the common remedies were employed to moderate these symptoms, with little or no benefit.

I thought myself justified in having recourse to any means, recommended by experience though unused in general practice, that offered a possible chance of relief in a case so desperate; and I therefore determined, "without being enlightened, I confess, by the grateful dawn of any probable theory" on the subject, to propose the inspiring of mephitic air, as mentioned on very respectable authority, in an appendix

dix to one of Dr. Priestley's volumes on air, to have been tried in similar cases, with some degree of success.

It was impossible to construct at sea such an apparatus as might have been wished, for the purpose of determining accurately the proportions of mephitic and atmospheric airs used in the experiment. Having however on board one of Dr. Nooth's glass machines for impregnating water with fixed air, I removed the upper part of it, as of no use for my purpose, and inserted a flexible tube, which I happened to have in my possession, through a cork, fitted to the superior orifice of the middle chamber of the machine, through which tube I meant my patient to inhale mephitic air. I filled this chamber of the machine nearly one-third full of pure water, with the view of arresting any particles of marble or vitriolic acid (the ingredients I used to obtain fixed air) which might be carried up along with the air from the lower chamber.

After the mephitic air had continued to ascend through the water, till I could perceive its peculiar odour issuing from the extremity of the flexible tube, I allowed my patient to take a full inspiration of it, and made him repeat the same, after an interval of one or two inhalations of atmospheric air between each, for a quarter of an hour or twenty minutes successively; taking care always to supply a brisk stream of mephitic air from below, by adding more marble and vitriolic acid when wanted. This operation was renewed three, four, and sometimes five times a-day; and no inconvenience or uneasy feeling was occasioned by it to the patient. On the contrary, he expressed himself somewhat relieved after it, and wished to repeat it oftener than I chose to venture. The cough seemed to be rendered less frequent and less violent; the matter expectorated assumed more of the consistence and appearance which denote laudable pus; the breathing became more free; and I thought the hectic fever was sensibly mitigated. Still however there was a progressive decay, and none of the symptoms were ever entirely suspended. The patient died after using the mephitic air, in the manner above described, for six or seven weeks, satisfied to the last that it contributed in a considerable degree to alleviate his sufferings. It is unnecessary for me to remark that before recourse was had to this remedy, the texture and even the substance of a great portion of the lungs were in all probability destroyed. It may be worthy of notice, what I had more than one opportunity to observe in this case, that the symptoms were milder ashore, and more disposed to be troublesome at sea, although the greatest part of our voyage



age was within the tropics, where of course no exposure to cold or to the common causes which increase the disease, could account for the circumstance.

The other case in which I employed the inhalation of mephitic air, was that of a lady aged about 22 years ; who nearly two years and a half ago, was seized in Russia with symptoms of a violent pleurisy, after incautiously eating iced cream when over-heated. Notwithstanding bloodlettings and other evacuations, the inflammatory symptoms seem to have run into a rapid suppuration, for eight or ten days after the first attack, and after a severe fit of coughing, almost immediate relief followed the sudden expectoration of a large quantity of what was deemed pure pus, slightly intermixed with blood. But though the pain and dyspnoea now abated, still a frequent cough and a very copious expectoration of a similar matter to that discharged at first, remained ; and soon her fever assumed a hectic form. She was in this situation recommended to come to England, but experienced no benefit either from the sea voyage or from the use of the Bristol hot waters, which she drank during some months. So much of her case I give from her own report. From Bristol she came to Bath in the beginning of last January, when I first saw her, eighteen months after the commencement of her illness. The state of circumstances then was, very considerable and *progressive* emaciation, an almost constant hectic flush on the countenance, the pulse always quick, with regular and strong exacerbations of fever towards evening, which again abated before morning, and were succeeded by profuse sweats ; the cough was very frequent, and the expectoration so profuse as completely to wet many handkerchiefs daily. She began now to inspire mephitic air, pretty nearly in the same manner as Col. Cathcart had formerly done. She not only repeated, however, the inhalations from the machine oftener, and continued them longer each time than was done in his case, but even while she was not inspiring through the tube, the machine generally remained on a table near her, emitting the fixed air which was continually extricated from the mixture of calcareous earth and vitriolic acid it contained, so that I seldom entered her apartment without perceiving mephitic fumes in a greater or less degree. The apartment being close and of no great extent, I sometimes thought it prudent to have a window opened for the purpose of clearing it of these fumes.—Particular circumstances rendered it necessary that I should inform the lady's relations without reserve, what chance I saw of her recovery ; and in the beginning of my attendance I did not hesitate to express my despair of doing her any good, or of ever seeing her better.

Such however was soon the abatement of all her symptoms under the above treatment ; so entirely for some weeks did the hectic fever disappear ; and so evidently did she gain during the same period both flesh and strength, that not only her relations acquired new and sanguine hopes of her recovery, but I began seriously to flatter myself with a disappointment of my predictions, although I durst not venture to avow it. The first check given to this amendment, which proceeded for four or five weeks, was occasioned by an over exertion of her lately recovered strength, during a fatiguing walk, the latter part of which was up a pretty steep ascent. A return of pain in the breast and dyspnoea, a tinge of blood in the expectoration, together with an accelerated pulse, made me have recourse to bloodletting, blisters applied to the chest, &c. which greatly relieved these symptoms, but at the same time reduced the general strength. The inhalation of mephitic air was interrupted during the period of this fresh inflammatory attack, from an uncertainty how it might act rather than from any observation of its disagreeing ; but it was repeated as before, after the symptoms of inflammation had abated, and again seemed to produce the same beneficial effects. A second relapse however occurred some weeks afterwards from a slight indiscretion, the throwing off part of her accustomed garments. This was removed much in the same way as the former one, and the mephitic air was again resorted to with similar success. After each of these inflammatory attacks, and after one or two others which happened subsequently, there remained for some time a considerable increase of cough and expectoration, and a permanent hectic, which however gradually abated under the use of the mephitic air. But these repeated relapses from slight causes, notwithstanding the constitution rallied astonishingly afterwards, and soon seemed to regain all it had lost, renewed my fears that the disease would soon run the usual and rapid course of confirmed phthisis. The patient left Bath in the month of May last, to take advantage of the summer season for trying another voyage by sea, still bent on continuing the inhalation of mephitic air. I despaired of hearing much longer any favourable accounts of her ; but have been repeatedly and agreeably disappointed, in learning that her health has since gained instead of losing strength. By a letter received within these few days from Petersburg, where she has passed the summer, it is reported to me “ that she is wonderfully recovered by the Balsam of Mecca, which she got from the Turkish ambassador.” Whether she has all along continued the mephitic air, I cannot undertake to assert ; but I believe in the affirmative, from her intentions

at



at the time of leaving this country. To whatever cause her preservation is owing, it is the first case of so fully formed, and so far advanced a phthisis that I have met with, in which the progress to dissolution has been so long restrained, or so successfully repelled.

I recommended to the parents of a young lady, who died of phthisis at Bristol about a year ago, to make trial of this method of exhibiting fixed air; and her father assured me afterwards that he had attempted it, but found her lungs could not bear it, as it excited irritation and coughing. From his account however I suspected that the operation was clumsily conducted, and that the coughing was produced by a stream of air rushing too suddenly from the tube into the fauces without an act of volition. Both of my patients experienced this inconvenience at first, owing to my very imperfect apparatus; but after they acquired the management of it, no such irritation was excited.

I shall be rejoiced to hear of your pursuing those inquiries on the effects of respiring different kinds of air, in which you have already displayed such happy invention in theory, with equal success in practice; and should the expectations suggested by an ingenious hypothesis be too sanguine, yet much advantage, I trust, will arise from this application of the recent discoveries in pneumatic chemistry, to the improvement of pathology and the cure of diseases.

I remain, dear Sir, &c.

*John Ewart.*

—Accounts from Petersburg of a late date state the amendment of this lady to be more considerable than I ventured in my last letter to represent it. It was her intention to pass the winter in the South of Russia, but she now thinks herself so well as to be able to remain with impunity at Petersburg. The expressions of her father in a letter to her sister are, “She has recovered progressively ever since she returned here, regains flesh and strength, is free from fever, and suffers very little from her cough, but continues to spit immoderately, though with ease.” No mention is made in this letter whether she persists in respiring fixible air.

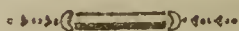
*Bath, Dec. 15, 1793.*

Your's, &c. J. E.

## LETTER

*From Doctor THORNTON,*

TO DR. BEDDOES.



SIR,

LONDON, *December 7, 1793.*

I SHALL be happy at all times to communicate to you the result of my various trials of factitious air. I more readily entered into your ideas, as to the change to be wrought on the blood by different airs, and consequently the removal of many otherwise incurable disorders, as I had formerly chosen for my thesis at Cambridge, "that all animal heat arises from the decomposition of air," or in other words, that the blood in the lungs receives from the air *oxygene in combination with matter of heat*:—and that in its passage through the body, the oxygene meeting with another superior attraction, forsakes the caloric; and the matter of heat being thus disengaged, (as with neutral salts whose basis is withdrawn from them) assumes its well known active character.

I was naturally led next to the consideration of the different mediums through which this matter of heat or caloric passes, and I found it pervade most rapidly bodies already saturated with oxygene.† In local inflammations I therefore forsook the old practice, and hastily withdrawing all oily applications, I substituted in their room the best conductors of heat. Having learnt from the experiments of Dr. Black, the different capacities for heat in water and steam, after long exercise I always recommended warm tea, or whatever else might produce perspiration. I was enabled to support the remarkable heats of last summer in a surprising manner by wearing a *fleecy hosiery waistcoat*; and since my first using this under garb, I am not subject to catch cold, as formerly, from vicissitudes of weather.

Four years ago I was at Bristol Hot-Wells, the sorrowful companion of a near relation, and I observed every consumptive person, I knew there, in time swept away by the *giant-malady* as Dr. Darwin most justly calls it. Being seized myself afterwards with every symptom of phthisis, it was likewise recommended to me to try the Bristol waters, but that my spirits might not get depressed, and not relishing repeated bleedings, the plan of cure then pursued, I retired to the Isle of Wight; and living chiefly on fried fish and animal food,

with



with much fresh butter, I recovered to the surprise of every one. This, Sir, was previous to your new method of treating that fatal disease, and before I could be biased by your theory.

Last September, when I went down to Pewsey in Wiltshire, I found my valuable and very learned friend, the Rev. Mr. Townshend, labouring under a most dreadful fever, such a fever as a few years back carried off 63 of his parishioners. His tongue was black; his breath putrid; his countenance sunk; several white specks were formed about his fauces; he had the subfultus tendinum and singultus; his pulse was quick and feeble. By the administration of bark and Port wine every two hours, and food in the intervals, these alarming symptoms vanished, but the difficulty of breathing still continued. Having opened the windows and sprinkled oxygenated wine (vinegar) like a fine dew over the apartment, the thermometer fell nearly four degrees, and the effect of a purer and colder air was such, that in a few moments after, he breathed as he styled it, like a sucking child, through his nostrils, and generally afterwards grew composed to sleep.\*

About ten days since, I was called to a patient, a child 13 years old; she had a fever, which had already attacked two other persons in the house. Mr. Murdock, the father of the child, told me that my medical skill could avail but little, as his child was at the point of death; and that all he expected from me was in some measure to palliate her sufferings. For three days and as many nights every thing taken into her stomach had been rejected. During this time she had had no sleep. Much watery liquor passed from her bowels, and she had an almost constant desire to go to stool. The two last nights glysters of mutton broth had been administered. When I entered the room she had just been convulsed, was speechless, and gasping for breath. Her eyes were fixed and sunk, and surrounded with a circle of a darkish brown colour. The muscles of the face still quivered. I immediately opened the window, for the room had but one, and ordered the fire to be put out. I removed some portion of the flannels, with which she was covered, and took off one blanket. I then administered factitious oxygene air, and to the astonishment of the beholders an acute pain in the left side first abated and then altogether ceased. Her speech was restored. As she seemed exhausted for want of food, I took the white of an egg, which of all nutritious substances I judged the least subject to corruption, and mixing it with white wine, warm  
water,

\* Mr. Townshend himself, the celebrated Spanish traveller, lately described to me the relief he experienced from the air of his apartment, charged with the fine spray of vinegar. The language he used was such as medical practitioners are accustomed to hear, when the patient is suddenly delivered from the most intense pain or distressing anxiety.

water, cinnamon, and afterwards with calves' foot jelly a little acidulated, I gave it her in small quantities, and finding that it remained, I soon after tried the bark and red wine, stopping whenever the least inclination to vomiting came on. The child was recovering fast by this treatment, when some officious female interfered. The consequence was, that the child was again seized with convulsions and became speechless. But in less than five minutes she was restored by breathing pure air. She is now out of danger, and doing well.

That hyper-oxygenated air is an admirable cosmetic, and the acquirement of colour attended with no diminution, but generally with an increase of health and spirits, I could adduce many respectable testimonies to shew. With electricity I make no doubt it will be found to be the most effectual cure for chlorosis.—The good and bad effects from the transfusion of blood (as formerly employed) may be now accounted for. As the post is on the eve of departing, I shall detain you with but one observation more. The caloric imparted from oxygenated blood appears to be the stimulus most essential to the animal œconomy. It is pleasing to observe that the power of being irritated in the nervous system keeps exact pace with the quantity of this stimulus generated in the animal body. Hence the reason of the long life of the heart of fishes, 'as it is called, and of all animals whose blood is cold. What advantage may be derived to the sick by increasing or diminishing this natural stimulus may be easily conceived!

I have the honour to be, &c.

*R. J. Thornton.*



*Extract of a letter from Dr. ———.\**

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I HAVE lately tried purified (*hyper-oxygenated*) air in the case of the ——— of Mr. ———, the celebrated surgeon in ———. This young lady has for two years been subject to repeated spasms, and has found no relief whatever from medicine. She has been considerably better since her first breathing purified air. Yesterday, just before she was to imbibe it, a spasm came on, such as terrified all around her. She had not breathed the portion of air I judged a proper dose, when to the astonishment of Mr. ———, surgeon, the spasm ceased.—I am daily more and more convinced of the justness of your ideas on consumption. I have been to Haslar Hospital to observe the sea scurvy.—A lady in the state of pregnancy, whom I have just left, is drinking vinegar, which she could not before bear. She imagines her frequent tooth-aches to proceed from the four apples she has lately so much indulged in. May not the qualms in pregnancy be removed by purified air?

Dec. 19, 1793.

Your's &amp;c. DEAR SIR

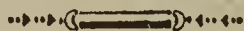
TO DR. BEDDOES.

\* I receive this letter at the moment of delivering the last parcel of MS. to the Printer. The communication was not made for the sake of publication, and I have not time to request permission and further particulars. So I must suppress names. But I may venture to assure the reader that he need not doubt the authenticity of the account a moment. To say nothing of the writer, the parties mentioned in the letter are, some of them, well known to the public. From the slight intimation given of the case, it appears to be one of those nervous affections where opium in large doses often does service.

*Case of Dyspnoea, approaching to Orthopnoea, in a letter from Benjamin Biggs, M. D.*



IT is proper to premise that the following case could not at the time Dr. B. was at Bristol be referred to any disease for which we have an appropriated denomination and definition in the common books of Nosology. It came nearest to what is called an humoural asthma; the nocturnal accessions of difficulty of breathing did not observe the course of the paroxysms of the true asthma; nor were they preceded by drowsiness, yawning and the other symptoms usually preceding such paroxysms; nor did they, like asthmatic fits, occur from time to time, leaving the patient free in the interval. Dr. B. inspired one part of oxygenic air mixed with three of atmospheric three times a-day at first, for five minutes at a time. He afterwards inspired the same mixture for twenty minutes at a time. Notwithstanding the effect of this process on the dyspnoea, the cough and expectoration, continued much as before.



DEAR SIR,

OCTOBER 14, 1793.

I HAVE for near two years been subject to a cough with spitting of mucus and very considerable difficulty of breathing, the attacks of which resembled asthmatic paroxysms in coming on in the night, often after my first sleep. They very frequently obliged me to rise out of bed and walk about the room. I was always forced to sleep with my head considerably raised. For these symptoms I had employed various antispasmodic remedies, which afforded only relief for the moment and not always that. After breathing the mixture of airs you directed, I found this difficulty of breathing much relieved in three days, and before the expiration of eight days, it had entirely ceased; and has never returned since.\* Before this time, I had been subject to coldness of the extremities, which now went off. I could even sleep with fewer bed-cloaths. I had also a greater flow of spirits.— I can hardly doubt, from my own feelings, that this kind of air will be highly beneficial in that very distressing disease, the asthma; and in diseases of languor also. I had tried various climates, the Bath and Bristol waters, in vain; I had consulted at least twelve physicians in Europe, the West Indies, and America. I am, &c.

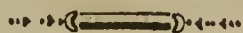
TO

DR. BEDDOES.

*Benjamin Biggs, M. D.*

\* This was written about three weeks after Dr. B. had ceased to inspire the mixed air.



*Case of Epileptic Affection.*

ABOUT three years ago a young man (aged 20) after an excursion on the heights of the Alps, during which it is probable he experienced some terror, was seized with a fit in the night. He had dreamed of falling from a precipice. There appeared evident marks of his having been strongly convulsed. This attack was at first considered as the nightmare; and valerian with other medicines, called *nervous*, were in vain administered. Sea-bathing disagreed with him; and cold bathing in fresh water rendered him suddenly worse, inasmuch that his fits, which at first occurred only once or twice a-week, increased to the number of 28 in 24 hours. They afterwards diminished in frequency, and for a long time have not exceeded 12 in the day and night. They differ in degree, if not in kind.—In the more violent, he is insensible. These fits continue from one to three minutes, and he is comatose for about ten minutes afterwards. They occur only in the night.—The slighter fits occur both in the day and night, but more frequently in the night. He has often only one or two by day, and eight or ten by night. These last from 10 to 15 seconds, during which time the patient is sensible and often speaks with perfect knowledge of what is passing, though somewhat indistinctly. The instant they are over he is quite well, or rather relieved. If he is seized while on his legs, he falls with force; many of his muscles become rigid and others convulsed. On his chair, he may have a fit without the knowledge of a person sitting in the same room. For a long time he was continually drowsy; he could neither look up to any height nor down from it; he could neither read a single line nor exert the smallest effort of attention, without bringing on a fit. But none of these circumstances now affect him. His appetite and spirits he has always retained; nor are his faculties impaired.

Having passed through the hands of many physicians, he had exhausted the *materia medica*. The last physician he had consulted, had conceived the design of putting a stop to his fits by large doses of opium, administered towards evening and during the night. The first two grains however of this drug produced a frantic delirium, which required the assistance of eight persons to secure the patient. This state of violent excitement or intoxication lasted 18 hours, during which indeed there was no fit: but the fits were rather more severe than usual during the weakness that followed. A subsequent trial of opium also failed.

Some time afterwards it was suggested that the inspiration of modified air might be serviceable. Upon being consulted concerning the probable success of this plan, I could give little encouragement. The only hope I conceived arose from an analogy which will presently be mentioned: Although therefore I believed we might manage so as not to do permanent mischief, I thought it due to the patient and myself to declare that the event might possibly be to a certain degree unfavourable. It is not surprising that this consideration should have been superseded by the wish to be delivered from so distressing a state.—No trial having been before made in a similar case with air containing either more or less oxygen than the atmosphere, I had only analogy for my guide. The following probabilities determined my choice.

1. Animals breathing air of too low a standard fall into convulsions.
2. The cold bath had permanently aggravated the complaint; but cold is only the abstraction of heat, and the abstraction of oxygen might, I feared, be prejudicial.
3. The great physician, who suggested the use of air of a reduced standard in the present case, observes in a work which will speedily be published: “If the excitability of the system depends on the quantity of oxygen absorbed by the lungs in respiration, sleeping in an atmosphere with less oxygen might be of great service in epileptic cases, and in cramp, and even in fits of the asthma, where their periods commence from the increase of irritability during sleep.” Now the lighter fits in the present case came on chiefly, perhaps in the proportion of 8 to 1, during sleep; and the severer fits always; and I had found that in asthma the nocturnal fits were prevented by air with excess of oxygen. This I thought a strong analogy.
4. I believed, and it appears still probable to me, that there is a great difference between tendency to spasm or convulsion, and strength in muscles: I hoped that oxygen, by strengthening Mr. —’s muscles, would diminish their too great mobility.
5. The patient is of that temperament, to which laxity of fibre is ascribed.
6. His youth was an objection to this mode of treatment, but he was rather fat for his age, whence I inferred that he had not already an over-proportion of oxygen in his system.

From these considerations he was desired to inspire a mixture of three parts of atmospheric and one part of oxygen air, for ten minutes on going to bed. As no effect was perceived, the time of inspiration was next night (Sunday night) extended to twenty minutes: after which he felt an agreeable glow in his chest. On Monday night at three intervals he inspired for half an hour: and by way of precaution a saline draught with antimonial wine was ordered for him, and his diet



diet was a little lowered. On Tuesday night he inspired for about twenty minutes : on Wednesday the air was omitted. On Thursday as no sensible effect followed, and as he passed good nights and had had no fit during two of the preceding days, the mixture of air was made a little stronger with oxygen, nearly as one of this species of air to two of atmospheric : He inspired for half an hour, and felt uncomfortably hot afterwards. In the morning his pulse was 72, and of natural strength. He coughed slightly, but found himself very well. He had no fit during the day, and the friend who accompanied him, and who had observed him with great attention ever since the commencement of his indisposition, thought him so much better than he had been for some time past, as to write a favourable account to his distant friends. Towards night he was unusually lively, but perfectly composed. This night the inspiration was omitted ; as I had originally determined to interpose an interval of several days, as soon as any effect, good or bad, should appear. He had scarce lain down when he was alarmed with what I conceive from his description to have been a starting of the abdominal muscles. This soon ceased, but I found him flushed and somewhat feverish, with a pulse above 100 and rather strong. He had a strong tendency to muscular motion, but was easily persuaded to lie quiet. He appeared as if a little intoxicated, and at the same time alarmed at his situation. One of his slighter fits supervening increased his apprehensions, for he had conceived some hopes that this would prove the crisis of his disease. As he had had no motion the preceding day, a gentle cathartic was prescribed and operated as was wished. During the course of the night he had a kind of drunken delirium, similar to that which opium had produced, only far milder and accompanied with singular muscular agitations : The toes sometimes moving like the fingers of a person playing on the harpsichord, and the lower extremities being frequently in action. But the motion of his arms was the most constant ; and this was of a very curious kind. It exactly imitated the gestures of a person who very gracefully drives a pair of horses from a phaeton. To this exercise the patient had long been accustomed for three or four hours every morning, but he had lately discontinued it for about a fortnight. These gestures lasted till some time on Monday, when all the other movements had subsided. He frequently declared them to be involuntary, and at breakfast on Monday, when he was quite composed, was rather amused with his own inability to restrain them. He had only five or six of the slighter fits in twenty-four hours ; but did not sleep till Saturday night, when he fell into a profound sleep and had the usual

usual number of fits with a delirious accession early on Sunday morning. Early on Monday morning, he had a similar, but much fainter paroxysm, which was the last. During the rest of Sunday night he slept as before very profoundly, which he had also done in the day-time. The muscular agitations were at this time gentler during sleep and confined to the fingers principally.—The muscles that move the joints had been so much in action as to produce that general stiffness and soreness which follows unusually severe exercise. The pulse soon became feeble, and was sometimes 108. He was full of apprehension during this whole time, but his fears gradually subsided. At times he appeared to be torpid.—It is remarkable that not only the prevailing state of the mind was the same during the action of the opium, but that it was occupied by the very same ideas on both occasions. The whole effect of the opium I was informed, totally disappeared in less than 40 hours; that of the oxygene air lasted 12 hours longer; the excitement of the sensorium was far more violent and continued in the former case; but this was compensated by the extraordinary muscular agitation in the latter case. This agitation has indeed been in a slight degree since observable during sleep; a gentleman, who lately watched the patient all night for the sake of making observations, has also had reason to believe that the same apprehensions, which he expressed during his periods of excitement, recur in his dreams. In his waking hours and in other respects he has appeared at least as well as before he inspired oxygene air.—I was not prepared to expect any thing like intoxication from an excess of oxygene, especially as in instances where I have known more inspired in the same time, nothing beyond a sensation similar to the alertness of healthy children was felt.—The muscular agitations of this patient contribute to render it probable that the difference of muscular irritability in different persons partly at least depends on a difference of oxygene in the muscles. This phenomenon is, in my opinion, to be classed with the increased vivacity of the system in animals that have respired air of an higher than the common standard. The present case shews also that if any one should attempt to restore or increase the irritability of his muscles by vital air, he ought to conduct the process very gradually; and perhaps in this way the progress of old age may be arrested: and much of that listlessness prevented, which renders the decline of life so comfortless and sad.

Excitement usually follows the application of intoxicating stimulants more speedily than in this instance. But supposing the muscular movements to have arisen from the increased proportion of oxygene in the muscular fibres, the blood

would

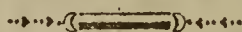


would take some time to feed them with this superabundant quantity; and perhaps the delirium was only symptomatic of the agitation; which took place first, and which, I am certain, did not proceed from any ordinary stimulant. The patient on that day, by my desire, had even dropped his ordinary allowance (four glasses) of wine. Any medical inferences that may be deducible from these facts, the reader shall draw for himself. For my own part, my want of success induces me to wish that I had followed the suggestion of the physician whose opinion I have quoted.

What was the immediate cause of these singular muscular movements? Could they be excited by the blood, rendered unusually stimulating, as it traversed the muscles, rendered unusually irritable—by an overcharge of oxygene?

I have already said that this case is now published by way of caution. As I am persuaded that many sick people, harassed by disease and tired of medicines, will themselves suggest the trial of elastic fluids, I hope the caution will be regarded. The credit of the *Digitalis* suffered from its being given in doses twenty times too large: so would that of opium, mercury, and antimony, if they were now first about to be introduced into the materia medica.—N. B. It is about seven weeks since this case occurred.

*Abstract of Mr. Vauquelin's experiments on the liver of the  
Ray or Skate fish.*



THE Skate has a very large liver in comparison with its heart and its organ of respiration. The liver in this fish is of a very fat nature, as is well known to cooks, who always observe that it affords, upon being boiled, a great deal of oil, which continues liquid in the ordinary temperature of the air.—Of skate's liver Mr. V. reduced 1 oz.  $4\frac{1}{2}$  gros (drachms) by pounding it in a mortar to a sort of pap, on the surface of which were seen to float particles of white oil; 4 oz. of cold distilled water readily combined with this pap; the mixture was whitish, and on adding more water, became as white as milk. The liquor being passed through a fine silk sieve, nothing remained behind but the investing membrane. In a few hours there appeared on this emulsion, a yellowish cream like that which is seen on diluted milk or on an emulsion of almonds; it was doubtless some of the oil which separates and carries up a little of the parenchyma. This milky liquor is decomposed by even the weakest acids, they produce curds or coagula, which rise to the surface, as when soap is decomposed by an acid.—The above-mentioned cream, being skimmed and agitated long in a mortar, did not yield butter like the cream of milk, but only an oil of a thicker consistence than that which was procured by heat in a subsequent experiment. Paper, on which the liver of a skate has lain, becomes transparent and oily.—4 oz. of liver, covered with its membrane, after being bruised, were heated slightly in a pipkin: on the first impression of the heat, a coagulation took place and much yellow oil separated; the heat was applied as long as any steam arose: then the oil was passed through fine linen, and a strong pressure applied to separate it from the parenchyma, which afterwards weighed 4 gros. 36 grains, but still retained a good deal of oil; the collected oil weighed 1 oz. 7 gros. these together make 2 oz. 3 gros. 36 grains; hence the water evaporated must have been 1 oz. 4 gros. 36 grains. The 4 gros. 36 grains of parenchyma afforded, on combustion, 8 grains of ashes, which proved to be phosphate of lime.—Upon 2 gros of oil from skate's liver there was poured oxygenated marine acid, till the acid ceased to lose its peculiar smell immediately. The oil became white like grease, but it had the ductility of wax that has been squeezed between the fingers. Mr. V. found that upon blowing on the surface of this oil, twelve hours after its expression, a white pellicle



pellicle was formed; this pellicle broke into fragments; which diffused themselves through the oil. When the oil was spread thin on glass, it soon became dense and opaque. "These experiments," adds Mr. V. "prove that skate's liver contains above half its weight of oil completely formed. The fluidity of this fatty matter shews the influence of the very inconsiderable respiration of this animal upon the consistence of its substance, and especially upon the character of its fat. The liver of man and of quadrupeds sometimes on being cut or torn, shews traces of oil, but it is far short of the quantity found in the class of swimming *amphibia*. In certain diseases of the abdomen, in diseases of the liver, this viscus swells, becomes almost white or greyish like that of the skate, and at length grows very oily. The livers of birds, especially of geese that are kept in a warm place and fed with milk, exhibit the same appearance. Probably the blood, passing through the system of the mesenteric, splenic and hepatic arteries, undergoes great changes; whether, as some physiologists with little probability suppose, it dissolves the abdominal fat, or whether, as I am inclined to think, as it slowly traverses these regions, the carbone it contains attracts to itself all the oxygene which had been only introduced among all its particles as it traversed the lungs; and hence the blood itself, immediately before it returns to the thorax, acquires from the superabundance of hydrogene an oily nature, which it imparts to the organs it nourishes. (a) If this takes place at all in man and quadrupeds that respire much, and in whose vessels the circulation of the blood is very rapid, it ought to be far more striking in those singular animals, which are capable of living long in mud or the most offensive mire, and which have a very limited respiration, since in proportion to their size their respiratory organs are very minute, and of course admit but very little air; which from the slowness of their circulation, is not imparted to their whole mass of fluids till long after it is received. Hence those animals are all soft and cartilaginous; pallid and colourless throughout, and little sensible or alert. I attribute therefore the pre-eminence of the liver in this class of animals to their deficiency of respiration; as also the liquid and oleaginous substance of their brain."—So far Mr. V.—I add 1. That if one of his last remarks be just, oxygene in some form or other may be expected to cure that strange disease, *the emollition of the bones*.

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(a) I intend to examine, whether the blood of the venous system of the abdomen, collected in the *vena portarum*, does not contain carbonic acid, or more carbonic acid than the rest of the blood, supposing this to contain any. V.

*bones.* 2. The whole tenour of these facts tends to confirm the connection between a certain deficiency of oxygen in the human system and the formation of fat. As animal chemistry is improved, the art of fattening animals will also be rendered more cheap and expeditious.—Should opulent people in general acquire a taste for knowledge, many experiments upon a large scale will be executed, of which the result will have a very salutary re-action upon medicine and physiology. For instance, if skates and other animals of this class were long kept in reservoirs of water in contact with oxygene air, their structure and, consequently their nature, would probably undergo a great change. 3. Dr. Withering has communicated to me a fact towards the confirmation of my conjecture, that fat is generated at the expence of muscle. In Portugal, where Dr. W. spent the last winter, hogs are fed much fatter than in England. He informs me that he observed in one instance in particular where the hog was more than ordinarily fat, that there was no muscle on the cheek, and very little on the ham.

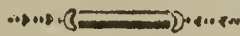


In the *Monthly Review* for November 1793, the following among many other acute remarks occurs: “It is assumed by Dr. Beddoes that the blood of pregnant women has a diminished proportion of oxygene; but pregnant women have the same circumscribed spot of florid acid in their countenances, which is apparent in hectic: if then the presence of this colour be sufficient to prove an excess of oxygene in the one case, it must have the same weight in the other.”

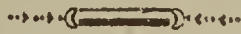
Undoubtedly, the theory cannot be put to a more proper test; for, if the flush of pregnancy and of florid consumption be the *same*, my opinion concerning the hyper-oxygenation of the system in florid consumption loses all the support it seemed to receive from this appearance: nor can the two states be opposite with respect to the proportion of oxygen. Let observation therefore decide.—To me it has appeared that the fixed spot on the cheek of pregnant women is dark-coloured in comparison with the fine crimson of the hectic flush: and I have thought it more nearly to resemble the complexion of some elderly people, or that hue which is occasioned by cold, both which I suppose are owing to a paralysis or relaxation of the cutaneous capillaries, particularly of the veins. The varicous swellings, vibices and livid spots in pregnancy seem to indicate a similar cause. It may at least as a friend has suggested to me, deserve to be considered whether these appearances are produced solely by mechanical



mechanical pressure.—Typhus, I am informed on good authority, has suspended consumption. This fact is perfectly consistent with my opinions. I originally expressed the most sanguine expectations from oxygene air in malignant fevers, and the opinion has been corroborated by facts.—It may be said, in the language of the late Mr. Hunter, that, in all these instances, phthisis is suspended by a *new action* being induced. The answer is true, no doubt; but we ought not to rest satisfied with it, since it leads to nothing useful in practice. But it would be useful to ascertain the nature of the new action or, at least its cause; and to this my inquiries have tended.



I once hoped that this collection would have been enriched by the case of Dr. JAMES CURRIE of Liverpool, drawn up by himself. Other occupations, however, have prevented Dr. Currie from executing his obliging intention towards me. As this ingenious physician laboured under the most alarming phthifical symptoms, the observations he made upon himself must be highly interesting, and it is to be hoped that he will one day make the particulars public. He has favoured me with the following summary. “ My case contains nothing that seems to me applicable to your theory. “ The sole inferences to be drawn from it are, that in the “ florid consumption a change of air from the sea-shore to an “ inland and mountainous situation is highly useful; and “ that the hectic paroxysm on its approach, may be prevented “ by the swing, in some instances, and by exercise on horse- “ back in still more, to which last, persevered in with a de- “ gree of pertinacity that is not common, I chiefly impute “ my own recovery.”

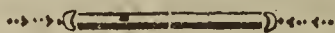


In a letter, dated Jan. 2, 1794, Dr. Thornton informs me of an instance of great and unexpected relief from the exhibition of atmospheric air mixed with a small proportion of hydrogen air in the last stage of consumption. “ Mr. “ C——ns” he says, “ besides the ordinary symptoms of con- “ sumption, had an oedematous swelling of his feet and “ ancles; that last fatal symptom, a laxity of his bowels, had “ even come on, Upon being called in, I resolved upon a “ very cautious trial of hydrogen air, which I employed at “ first in not more than the proportion of one part of hydro- “ gen air to thirty of atmospheric air. Under this treatment, “ to my great surprize, he has gained strength; the diarrhoea “ ceased and the oedematous swelling disappeared. And it “ deserves to be mentioned, that some particular circum-

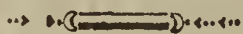
stances having at times prevented his being supplied with the mixed air for several days, he has become worse, and gained ground, after inspiring it again." Dr. T. adds at the close of his letter this just observation—"Should any practitioner, bold through ignorance, do essential injury by an injudicious administration of air, the unhappy event would be blazed through the kingdom; and the benefit that will otherwise probably result to mankind from your proposal, perhaps be excluded for ever." The same chance of falling into unmerited discredit awaits every substance endued with active properties, on its first introduction into medicine.



*THE following communications are not noticed in the table of contents, because they were received after that table was printed off. The publication of the pamphlet was delayed in order for their reception. They tend to shew that the administration of factitious airs in certain diseases is SAFE and PROMISES ADVANTAGE to Society; and that the design ought to be prosecuted, which is all that I maintain in this or any preceding publication. T. B.*



## SECOND LETTER from DR. THORNTON.



DEAR SIR,

Great Russel-street, Bloomsbury,  
Jan. 4, 1794.

**I** HAVE lately found vital air of great use in the removal and alleviation of certain spasmodic diseases, as the asthma and hooping-cough. One spasmodic case, that came under my immediate care, deserves, I think, your particular attention.

An amiable young lady, nearly related to some gentlemen of the first eminence in the medical world, has been for the last two years dreadfully afflicted with violent spasmodic seizures. Opium had been largely administered, but it ceased to have the desired effect. Nothing gave relief but water impregnated with carbonic acid air. Previous to her second trial of breathing a purer atmosphere, a violent spasmodic seizure came on, which seemed particularly to affect the diaphragm. All who were around her were alarmed. Her brother-in-law instantly urged me to administer the medicinal air. She had scarce breathed it three minutes, when to the surprise of all who were present, the spasm left her. It returned however with diminished violence. From daily



daily breathing certain portions of this elegant, and safe remedy (it judiciously administered), she has had fewer attacks; and these less violent, and much shorter in their duration.

I cannot at this time forbear mentioning the instance of a clergyman, who laboured under dyspepsia and depression of spirits. He had taken the tincture of bark without experiencing much benefit. As nothing conduces more towards good spirits and digestion than a clear pure air, I administered the vital air blended with atmospheric. The load on his chest, as he called it, was removed. His appetite was quickened. His spirits were raised even to the pitch, I call, gaiety; and as he informed me, he felt a strong inclination to go to the play, to which he had not been this winter, and he says he is fully convinced that no inducement could have got him thither, had he not previously breathed a more exalted atmosphere. Languor, you know, listlessness and inactivity are characters of hypochondriasis; and dyspepsia is its frequent attendant.

Concerning the other kind of air, whose properties are diametrically opposite to the last, I am now administering it to a gentleman, who when he came to me, appeared greatly emaciated from consumption: his cough was troublesome, his voice was gone, his ancles were swelled, and a diarrhoea was on him, which last symptom towards the close of this disease baffles the power of every known medicine. He was uncommonly weak—but his appetite, as often happens, was good.—As flannel frets the skin, however the impression may be weakened by repetition, yet as exciting the system without any just reason, I recommended it to him to change his flannel for fleecy hosiery, which equally with flannel absorbs the perspiration, and as being a bad conductor of heat, hinders us from feeling any changes of the weather. For milk in the morning and at night, I substituted patent cocoa, and some slices of cold boiled leg of pork. I advised for dinner instead of vegetables meat well done, chiefly mutton chops, and French bread. His medicines were such as attract oxygen, as a moderate use of wine, opium, and almond milk, with the addition of oil of almonds, of which he took a great quantity in the day. I desired him to avoid whatever tended to oxygenate the blood, as strong exercise, acids, &c. and he breathed at first atmospheric mixed with hydrogen air. Afterwards I preferred azot, combined in a certain proportion with atmospheric air; the result was, these very formidable symptoms soon disappeared, and what makes me give some share of credit to the air, is, that when he has left off breathing it for a few days, he finds himself worse; and

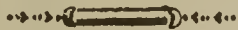
and he always declares himself better, when he has breathed it again for a few days successively. I am in great hopes the sequel of this case will prove as flattering as the commencement. In the last letter I wrote to you, I mentioned some cases, in which I was about to try your medicinal airs. I am now waiting for an opportunity to employ the oxygen air, for the immediate recovery of persons in syncope.—Would not this air, my dear Sir, be found of great service if it were let loose in mines, in churches, and in crowded rooms, but more especially in the bathing-rooms at Bath, where great faintness is often brought on the patient by breathing a reduced atmosphere from the extrication of azot out of those waters.

I am, &c.

*R. J. Thornton.*



*On the use of Yeast in putrid fevers.* By the Rev.  
Edmund Cartwright.



*A copy of this paper was first sent to Dr. Pegge at Oxford, at the Doctor's desire. The author, afterwards hearing that the communication might be acceptable to me, very obligingly and humanely transmitted it without delay.*

T. B.



ABOUT seventeen years ago I went to reside at Brampton, a very populous village near Chesterfield. I had not been there many months before a putrid fever broke out. Finding by far the greater part of my new parishioners much too poor to afford themselves medical assistance, I undertook, by the help of such books on the subject of medicine as happened to be in my possession, to prescribe for them. In the course of my practice I attended a boy about 14 years of age, who was attacked by a fever: what its appearances were in the first stage of it I forget. He had not been ill many days before the symptoms were unequivocally putrid. I then administered bark, wine, and such other remedies as my books directed. My exertions, however, were of no avail; his disorder grew every day more untractable and malignant, so that for more than a week I was in hourly expectation of his dissolution. Being under the necessity of taking a journey, before I set off I went to see him, as I thought, for the last time; not, indeed, with the slightest degree of hope to be of service to him, but solely for the purpose of preparing his parents for the event of his death, which I considered as inevitable, and of reconciling them, in the best manner I was able, to a loss which, I knew, they would feel severely. While I was in conversation on this distressing subject with his mother, I observed in a corner of the cottage a small tub of wort working. The sight brought to my recollection an experiment I had somewhere met with, of a piece of stale meat being made sweet by being suspended over a tub of wort in the like act of fermentation. The idea instantly flashed upon my mind that yeast might possibly be of service to my patient: without a moment's pause or reflection I gave him two large spoonfuls. I then told the mother, if she found him no worse for what I had given him, to repeat the dose every three hours. I then took my leave, somewhat precipitately, I own; for I began to think it possible the yeast might ferment so violently as to bring on an immediate suffocation. I set off upon my journey, and was absent about a fortnight. Being told on my return the

boy

boy was recovered, I could not repress my curiosity to see him immediately. Though fatigued with my journey, and night was coming on, I went directly to where he lived, which was three miles from my house, in a wild part of the moors. I found the boy, as I had been told, perfectly well. On inquiring of his mother the manner and progress of his recovery, she told me, I was scarcely out of sight before the boy said to her, "mother, I think I am getting well:" and from that time he continued to mend as fast as possible. The success of this experiment determined me in every case of fever, not obviously inflammatory, to administer yeast, not omitting at the same time such other remedies as the nature of the disorder might seem to call for. In the space of two years afterwards, while I continued my residence at Brampton, I make no doubt I attended nearly fifty poor people in fevers of the low putrid kind. What will appear singular, I did not lose one patient in all that time. It is to be observed, however, I had an advantage which more regular practitioners have not; as my advice and remedies were administered gratis, I was usually consulted on the first attack of the disorder, so that its progress was stopped before it had time to become so dangerous as otherwise it might have done.

After I left Brampton I went to live in Leicestershire. My parishioners there being few and opulent, I dropped my medical character entirely, and did not even prescribe for my own family. One of my domestics falling ill, the apothecary was sent for. His complaint was a fever, which in its progress became putrid. Having great reliance, and I believe with reason, on the apothecary's penetration and judgment, the man was left solely to his management. His disorder kept daily gaining ground, till at length the apothecary considered him in very great danger. At last finding every effort to be of service to him baffled, he told me he considered it as a lost case, and that, in his opinion, the man could not survive four and twenty hours. On the apothecary thus giving him up, I determined to try the effects of yeast. I gave him two large spoonfuls. Recollecting the very sudden effect I was told it had on the first patient I administered it to, I laid my watch upon the table, and took the man's pulse into my hand. In about ten minutes I perceived an alteration in it sensibly for the better. I then asked the man if the medicine I had given affected him in any particular manner, such as making him sick, disordering his bowels, &c. his answer, which I give in his own words, was strikingly emphatical and expressive; "I perceive no effect it has, but to make me feel *strangely lightsome*." In fifteen minutes from taking the yeast, his pulse, though still feeble,



feeble, began to get composed and even. He then observed, that not having been out of bed for many days, it would be great refreshment to him to get up, if only for the purpose of having his bed made. In thirty-two minutes from his taking the yeast he was dressed, and walking about his room. At the expiration of the first hour I gave him a dose of bark in a glass of wine, which I washed down with a quarter of a pint more. At the expiration of the second hour I gave him a basin of sago, with a good deal of lemon, wine, and ginger in it; he eat it with the appetite of a man in health: in another hour I repeated the yeast: an hour afterwards I gave the bark as before: at the next hour he had food of some kind or other; but what I do not now recollect; at the third hour, which was nine o'clock at night, he had another dose of yeast, and then went to bed. I went to him the next morning at six o'clock; he told me he had had a good night, and that he felt himself perfectly well. I, however, gave him another dose of yeast. He then got up, and went about his business as usual.

About a year after this, as I was riding past a detached farm-house at the outskirts of the village, I observed the farmer's daughter standing at the door, apparently in great affliction. On inquiring into the cause of her distress, she told me her father was dying. I dismounted and went into the house to see him. I found him in the last stage of a putrid fever; his tongue was black, a sanious ichor was oozing out of the corners of his mouth, his pulse was scarcely perceptible, and he lay stretched out, like a corpse, in a state of drowsy insensibility. I immediately procured some yeast, which, being stale, and consequently thick, I diluted with a little warm water to make it potable, and also to set it into a fermentation, and poured it down his throat. I then left him with little hope, as reasonably may be imagined, of his recovery. I returned in about an hour and found him perfectly sensible and able to converse. I inquired of him the effects of the medicine. The precise words he made use of I forget; his answer, however, was exactly to the same effect as the answer to the like question my servant gave. I then gave him a dose of bark. He afterwards took, at a proper interval, some refreshment. I staid with him till he repeated the yeast, and then left him, with directions for him to be treated in the same manner as I had treated my servant. I called upon him the next morning at nine o'clock. I found him apparently well, walking in his garden. He was an old man, upwards of seventy, of a thin spare habit. He was alive last year, and then nearly ninety years old.

About

About a year and half ago, a gentleman's son, in the neighbourhood of Doncaster, was attacked by a putrid sore throat and fever. He had been ill and in considerable danger before I heard, which was by accident, the nature of his complaint. I immediately communicated the above facts to the apothecary who attended him. It happened his disorder the evening before had taken a favourable turn, and consequently a change of medicine would not have been justifiable. In the course, however, of a few days, the nurse-maid, who waited upon the child, was seized with the same complaint, and was treated in the same manner, but with different success. The apothecary then gave the yeast. She recovered with a degree of rapidity which he told me he should have considered as incredible had he not been an eye-witness of it.

Though the very speedy operation of the yeast in all the cases I have enumerated may at first sight appear singular, yet if we consider the principle upon which it operates, it is reasonable to conclude, whatever its operation may be, it must be immediate, as it will begin to part with its fixed air almost as soon as it is received into the warm stomach.

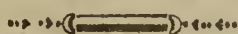
In cases of external mortification it might be applied to the part affected, as well as given internally. It probably might be found of service in cancers, if what Dr. Buchan affirms be true, that by means of antiseptics alone he kept a confirmed cancer at bay for some years.

In corroboration of the above facts, relative to the medical virtues of the yeast, I add the following one, communicated to me by Mr. Williams, a respectable clergyman of Pinner in Middlesex, to whom I had been mentioning the success with which I had administered that remedy.

When a young boy he was seized with the small-pox, and was thought in imminent danger. By the advice of an old Welch clergyman, who visited at his father's, he drank a hearty draught of beer out of the vat, the yeast being previously beaten in. His bad symptoms very soon disappeared, the pustules rose kindly, and he got through the disorder, in every respect, in the most favourable manner. Mr. Williams perfectly recollects his recovery being always spoken of in the family as owing to the prescription of their Welch friend. This event must have happened not less than forty years ago.



*Case communicated by Dr. PARRY, of BATH.*



WHEN the following case occurred, the pneumatic chemistry had been reduced to no systematic form. It had indeed long been known that blood was capable of becoming red by contact with atmospherical air; and Dr. Priestley had found that this happened even though the substance of a bladder was interposed, and that the change was most speedy and considerable when the experiment was made with dephlogisticated air. But although from these and other facts Dr. Priestley had drawn a very important conclusion as to the use of respiration, yet the application of this branch of chemistry to physiology had not much occupied the general mind. Hence it arose that some symptoms of the case which I am going to describe, and which was preserved principally with a view to investigate the nature of the hydrocephalus internus, were not so minutely related as they would have been had the inquiry taken a different turn, or had its object been more general. With regard also to the dissection, a very great embarrassment was thrown in our way by a strange tenderness of the patient's friends, who chose to have a man servant continue in the room during the anatomical examination. This obliged us to content ourselves with what imperfect information we could gain by hastily examining the heart in situ; and prevents my speaking with positiveness as to the non-existence of a canalis arteriosus, the number of the pulmonary veins, and some other circumstances.

On the whole however, this case, imperfect as it is, is one of the very few in which the mal-conformation of the pulmonary vessels affords a strong presumption that the red colour of the blood is owing to the oxygen which it receives during the act of inspiration.

The Hon. Miss V. was first put under my care in the spring of the year 1786. She was then between 13 and 14 years of age, of a placid temper, moderately tall, thin, and of a small make. The most striking appearance of deviation from the healthy state of the constitution was a lividness or bluish purple hue, which in some degree affected the whole skin, but was most intense where the tinge of the blood is usually most apparent, as in the cheeks, the nostrils, the lips, the ends of the fingers beneath the nails, and other similar parts. She constantly suffered more or less of palpitation of the heart, irregularity of the pulse, and hurried respiration; and these symptoms were much aggravated by any muscular exertion, though of the slightest kind, but became extremely painful in consequence of any stronger exercise. From

going up stairs, however gently, the livid blood became accumulated about the face and head, the pulse was accelerated to 120 or 130 beats in a minute, the irregularity of pulsation which I have described became more apparent, and a very quick and laborious respiration was induced. At all times, but more especially after the exertion of walking, the beating of the heart was more distinctly felt on the right than on the left side of the thorax. There was also this peculiarity in the circulation, produced, so far as I could find, by muscular exertion only, that the pulsation of the right carotid artery was very perceptible to the touch, while that of the left was extremely obscure; and that the number of pulsations in the left radial artery was smaller than of those in the right; the stroke in the former being sometimes missed, and at other times imperfectly performed, while the corresponding pulsation in the latter was distinct and strong.

The symptoms of disease which I have described came on without any obvious cause; nor could the young lady's parents ascertain at what period they had first been observed. This however was certain, that they had been considerably aggravated during the six years last preceding.

In addition to these complaints, Miss V. was occasionally liable to head-achs, evidently connected with flatulency, costiveness, and other marks of indigestion, which sometimes went so far as to produce sickness and vomiting. Emetics had therefore occasionally been given; and her mother was of opinion that they had, for some time, relieved her stomach complaints, head-ach, and dyspnoea.

Miss V. had never had the catamenia. Her appetite was tolerably good, and she was free from cough.

On the lower part of the os frontis, about the middle of the forehead, there was a tumor as large as a pigeon's egg cut through the shortest diameter, hard, immovable, and of the same colour and sensibility as the skin near it.

I regret that I made no memorandum as to the heat of her skin, or the proportion which the number of respirations bore to that of her pulse.

It required no great medical acuteness to discover that in this case there was a considerable deviation from the proper structure of the large vessels about the heart. I therefore apprized the friends of the patient, that nothing more could be done than to alleviate symptoms which would probably one day prove fatal. I desired that all violent exertion should be carefully shunned, but that gentle exercise, especially on horseback, should be assiduously used. At the same time Miss V. was advised to abstain from all full meals, and from every sort of food which could produce plethora or flatulency;



lency ; to clothe herself warmly, especially about her legs and feet, and constantly to remove costiveness by means of an aloetic pill. To these measures was added the internal use of small quantities of the Bath water.

This plan, continued more or less through the winter, in a great degree removed the symptoms of dyspepsia, and somewhat lessened those of undue circulation.

As the spring advanced, her friends wished to fix on some situation beneficial to her health, in which she might spend the summer ; but previously to any decision on this subject they complied with my earnest request that they would go to London for the purpose of consulting Mr. Hunter, from whom, in the month of April, 1787, I received the following letter.

“ DEAR SIR,

“ I HAD the honour to see your patient Miss V. There is certainly either disease about the heart and lungs, or an original bad formation of these parts, the last of which I am most inclined to believe. If the blood passes through the lungs without receiving the benefit of the air, it will come back to the heart venal blood ; or if it has any collateral passage into the aorta, before it passes to the lungs, in either case the parts where the blood is exposed will be livid ; and as this has been more or less a symptom ever since she has been born, it is natural to conclude it to be owing to a natural formation of parts. A case of that kind is published in the Medical Transactions of the College by Dr. Pulteney, which I saw, and where the symptoms were very similar to Miss V.'s, only, I think, more violent. However, as I can conceive disease, and most probably the scrofula, to produce similar symptoms, I think she should not lose any chance of being relieved on such an idea. After saying that every thing which increases the symptoms should be avoided, and that every thing which serves to keep them quiet should be strictly adhered to, I proposed her bathing in the sea-water. But it should be made so warm as not to give the least shock at first going in ; and this to be pursued according to circumstances.

“ As this is a case which, I think, will terminate ill, I wish you would take notes of all the symptoms ; for probably a time will come when the parts will be inspected by somebody, which will afford valuable information when attended by the history of the case. The pulsation being in one arm and not always in the other at the same time, is a curious fact, &c.”

Not long after this Miss V. went to the sea-coast and having pursued the plan which had been recommended, returned to Bath in the autumn, in much the same state of health as when she left it. During the beginning of the winter I saw her three or four times, but found no alteration in the symptoms.

On

On the 4th of January, 1787, I was sent for, and visiting her at two o'clock in the afternoon, found her lying in bed. She had complained for two or three days of pain in her head and some diminution of appetite, and on the evening before had been seized with vomiting, which had continued more or less till the time I saw her. She had taken no food, and had not slept during the night. What she had vomited an hour before my visit was fluid, slightly green, and of an acid smell. The pain of her head was not violent, nor, so far as I learnt, was it confined to any particular spot. Her tongue was slightly furred, there was no unusual appearance about the eyes, and her pulse, heat, colour, and respiration seemed to be in their natural state. During two or three days immediately succeeding, she had been constive; and had taken at the beginning some magnesia, which had produced scarcely any sensible effect on her bowels, and had not relieved any of the symptoms. She was ordered to take a scruple of calcined magnesia every hour till it operated.

At eight o'clock I found that four doses of the magnesia had been given without moving the bowels; but the vomiting had ceased after the first dose. It was said also that she had been asleep two hours, for which reason Lady — begged that I would not disturb her by then going into her chamber, but told me, that, previously to her going to sleep, Miss V. had been delirious, and attempted to get out of bed. I ordered a purgative glyster to be injected, and the magnesia to be continued.

Repeating my visit at ten o'clock at night, I was informed that the glyster had been imperfectly injected, without effect, and that she still continued to sleep. I begged however to see her, and found, on attempting to rouse her, that she was almost senseless. She was not affected by any noise, but seemed uneasy when a candle was brought near her, and the pupils were much dilated. She made an inarticulate sound with her voice, sat up of herself, and attempted to get out of bed, but seemed to have no consciousness of what was passing around her. Her respiration was quick and laborious, her skin in general rather hot, her feet cold, her face pale, and her pulse upwards of a hundred in a minute, extremely full, hard and labouring.

By my desire Dr. Falconer was called into consultation, and we met at half past eleven at night, when all the symptoms last described continued, but in a greater degree.

She was ordered to lose four ounces of blood, to have the purgative glyster repeated, to take a draught with a few drops of tinctura thebaica, to have the feet, legs and abdomen fomented with tepid water, and afterwards sinapisms applied to the feet.

Previously



Previously to the bleeding she had two fits, in which she was convulsed in various parts of the body, and particularly about the throat, as in the hysteria, and cried out with great violence. The glyster brought away at first a considerable quantity of hardened *faeces*, and afterwards a copious loose stool. After this evacuation and the bleeding, she seemed somewhat relieved with regard to her breathing and power of sensation.

The glyster was ordered to be repeated, and some broth occasionally given.

Jan. 5. Eight in the morning. She had had an evacuation from the glyster, but had passed a very bad night. Her breathing was still more laborious, her skin very hot; her pulse 136 in a minute, extremely strong and hard; the pulsation of the right carotid unusually full and bounding, her face still pale, but the pupils more dilated. Now also, for the first time, there appeared considerable strabismus. No urine had been made for upwards of twenty-four hours.

A repetition of the bleeding was ordered, and it was directed that she should be put into a tepid bath.

She was bled, and the bath employed, notwithstanding a considerable quantity of urine had previously been made. She then seemed again easier; but the symptoms soon rapidly increased, and at night she died.

On opening the body the following day, the cranium appeared to be unusually hard. The swelling in the forehead was found to be a tumor or thickening of the *os frontis* itself, which when the scalp was removed, was rough and of a livid spotted appearance, as from divided varicose vessels. The same rough tumor extended itself the inside of the cranium; but no particular disease was observable in the *dura mater* lining that part, or in the portion of the *cerebrum* immediately under it.

The *dura mater* itself was very tough and adhered strongly to the cranium.

The vessels of the *pia mater* were extremely turgid with blood.

A considerable quantity of water was found in the right lateral ventricle.

On the right side of the thorax, the ribs were very much depressed, while those on the left were in a natural state. The lungs on the left side were free from adhesion and any other appearance of disease. The right lobe of the lungs was so thin as to resemble nothing more than a plexus of membranes, it strongly adhered to the *pleura costalis*; but was free from tubercles, inflammation, or suppuration. The right pulmonary artery was small in proportion to the defect of the right lobe of the lungs.

The

The heart was considerably larger than natural, and the coronary arteries were full of blood.

Every thing about the arch of the aorta, and the carotid arteries, was in a natural state; nor was there any other unusual appearance about the heart or large vessels.

The mentum was void of fat; but all the viscera of the abdomen were free from disease.

*C. H. Parry.*

BATH, Jan. 17, 1794.



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